Ungerleider, Leslie 2002

Dr. Leslie Ungerleider Oral History 2002

Download the PDF: Ungerleider_Leslie_Oral_History_2002 (PDF 156 kB)

Leslie Ungerleider

This is an interview with Dr. Leslie Ungerleider, Chief of the laboratory of Brain and Cognition of the NIMH Intramural Research Program held on February 12th, 2002, in Bethesda, MD.

The interviewer is Dr. Ingrid Farreras of the NIH History Office.

Farreras: I wanted to thank you for meeting with you for this oral history. As I mentioned to you earlier, I am interested in your experiences with the former laboratory of Psychology as well as, more broadly, in you as an NIH scientist so I thought we could start with the earlier years of your life, where you were born, what kind of schooling you had, that type of thing…

Ungerleider: I grew up in New York City, in Queens. I'm from a middle-class family. I spent all of my childhood in and around New York City. I was academically bright and motivated. I went to New York City public schools. I graduated from high school at 16 and went off to college. At that time it was called Harper College but it was later renamed the State University of New York at Binghamton. In my first year, I took an introductory course in psychology, which at SUNY-Binghamton turned out to be largely experimental psychology, and I just fell in love with the field. Most other people were turned off by experimental psychology and went off to learn about personality and clinical disorders, but this was really interesting to me. It was after I left that Binghamton really started expanding the faculty and hiring prominent people. At the time, the school had about 2,000 students. It was a really small, liberal arts college, with no graduate programs, where everybody knew everyone, and so it was a very nice environment in which to grow and thrive and take interesting classes.

And then I applied to graduate school in experimental psychology and basically concentrated on schools in the New York area. I was an idiot. I knew nothing about why one chose one graduate program over another. Today, the students who come into my lab out of college, spend two years here before they apply to graduate school, and they have thought about and are very well educated about what the whole graduate school business is about and which schools to target. I didn't even know that you go to graduate school depending on who's there to work with and that that was the reason for choosing one school over another. I didn't even know that much. All I knew was that I wanted to be back in New York City. I was accepted at every program I applied in New York and NYU just looked interesting. I had no idea whom I would be working with.

In my senior year in college I discovered biology, and for a brief, fleeting moment, I thought about going to medical school. But then I decided I just didn't have the time. I was only 20 years old, but I decided I didn't have the time to go

back and pick up all my pre-med classes. But even then, it was clear that I was gravitating more toward brain science and less straight experimental psychology.

Now, my first year at NYU was a disaster. I had a miserable year. I was hooked up with a supervisor who did EEG work and event-related potential scalp recording, and I hated it, I really hated it. I told Murray Glanzer, who was the chair of experimental psychology at the time that if he assigned me to this man again, I would leave school, and I seriously considered doing that. And when I came back after my first summer and discovered that I had been reassigned to this man – I won't name his name – I stormed into Glanzer's office and told him that I was quitting unless he reassigned me to the person who ultimately became my Ph.D. advisor, Ted Koons, who turned out to be a wonderful, marvelous mentor.

Farreras: When you were assigned, was this as a research assistant where you had to work part-time on his research or…?

Ungerleider: No, I had a full stipend. The way the program worked at the time – I don't know if it still works like this – is that they tried to match you up with someone they thought had interests similar to yours. But he was doing human perception and scalp recordings, and this was not what I wanted to do. I really wanted to do animal work, in an animal laboratory, related to what I consider real brain science.

Farreras: So you already knew by then that you wanted to work with animal subjects versus human subjects.

Ungerleider: Yes, yes. And after I started working with Ted Koons, life became idyllic.

Farreras: What type of work were you doing with him?

Ungerleider: It was doing hypothalamic stimulation work, feeding, drinking, and reward systems, and it was just a wonderful laboratory. The morale in the laboratory was great. Everyone adored Ted. He was in the lab working alongside the students. Everything about it was wonderful, doing surgery on the rats, correlating behavior with brain stimulation parameters… It was just great, just great.

Farreras: Was he the one who trained everyone to do the surgery, or did he have assistants?

Ungerleider: Well, there was a mentoring system where the older students trained the new students coming into the lab. But there were frequent meetings where he met with us. It was a very, very close-knit group and we all got very close mentoring by Ted, and it just made graduate school wonderful. So I got my degree in four years and then started looking for a post-doc; this was 1970. My husband at the time was finishing his medical school training and did not get picked for one of the Berry plans and was called up by the Indian Service. There was a name for it. And they asked him whether instead of going into the military, he would put two years in either an Indian reservation or some Indian facility. The first job they offered him was in North Dakota and he said, "I cannot go to North Dakota because my wife will divorce me." And so the second program they offered him was in Oklahoma City, and in Oklahoma, they don't actually have reservations, but they do have health facilities for Native Americans, who can go to health clinics and be served by doctors in the Indian Service. It was called the Indian Health Service, so we moved to Oklahoma City, and I got a faculty job at Oklahoma City University for two years, where I set up a little rat lab and had undergraduate students doing all different sorts of experiments.

Farreras: Were you able to conduct much research while you were there?

Ungerleider: I was. I would say the quality of the research was at the undergraduate level in the sense that it probably was not anything that I would have published. But these were independent projects that undergraduates did with me as part of their honors thesis. And I got enough space to set up a little rat colony, and I went to talk to a breeder in town who was breeding Norwegian wild rats and we bought rats from him. And I was given maybe $2,000 by the university to do some stuff. I think it was a really valuable thing for the students and it kept me from going crazy. But after the two years we wanted to get out of there as fast as possible, and so I applied for a post-doc. We had to coordinate where we were going to be, and we decided that since we were so far across the country, let's just continue, and so we targeted California. My husband applied for residencies there in pediatrics, at Stanford, and I'm not sure if he applied to other places, so I got in touch with a faculty member at Stanford, Leo Ganz, and I wrote up an NIH extramural fellowship, what is now called an NRSA. I forget what it was called then. But I do remember it paid $7,000, which was pretty sad. I got one of these extramural fellowships, and we left for Stanford. But within the first year, it was clear to me that the kinds of things that Leo Ganz was working on were not the kinds of things that I was really interested in. It was low-level vision, and it was mainly in cats. But on the same floor there was Karl Pribram's group, and they were doing primate work and high-level vision and lesion experiments, and I thought, this is what I want to do. And so I switched mentors.

Farreras: How did you come across Leo Ganz in the first place?

Ungerleider: I knew Leo. How did I know him…I just can't even remember how I knew him.

Farreras: All right. I just thought that if you hadn't known him that you might have looked to see who was at Stanford and found Pribram's group as something more tailored to your interests…for your fellowship.

Ungerleider: My life is a big accident. It's like stumbling through life. I mean, why would I go to Stanford? I went there because at the time I was trying to work out something with my husband so we could be at the same place together, and I was really trying to jump-start my career again. I had been out of it for two years. So it was an opportunity to switch to more high-level vision work rather than hypothalamic work. I had looked at Leo's papers and they looked interesting, and I had gone out to interview with Leo and we seemed to get along, so the whole thing seemed like a good idea.

Farreras: And you were clearly interested in a research career, not a teaching career?

Ungerleider: Absolutely. There was no way that I wanted to continue being on the faculty of a small teaching college. That was just not for me. My goal was to get an academic position in a university and teach and do research, but the only way I would get a good academic position was to get back into research.

So I had three really great years at Stanford. In my third and last year at Stanford I went to the Society for Neuroscience meeting in St. Louis and presented some of the data that I was collecting with Karl and Mort [Mishkin] was in the audience. Just for background, Karl Pribram and Mort Mishkin, for many years, had been scientific adversaries about the role of pre-striate cortex, the role of the inferior temporal cortex, what their contributions to perception are…

After the talk Mort came up to me and said, "You know, this is completely contradictory to some of the results that we're getting," and we spent a long time talking. And Mort said, "Well, why don't you come to NIMH and we can try to sort this out." And, it was the most wonderful opportunity. I was, at the time, actually looking for jobs. I was on the job market but concentrating in the Northern California area because my ex-husband had an opportunity to do a fellowship in pediatric oncology at Stanford and wanted to stay there. So I was interviewing at places like San Jose State but never got job offers. And they would always say to me, "You're terrific, but this is a bad fit. You are not going to be happy at our institution." So I returned from St. Louis and told my then-husband that I got this offer at NIMH and I needed to go there. So he arranged to come here to the National Cancer Institute as a clinical fellow. He trained under Phil Peazo, who became a big figure and is now at Stanford, I think, and someone else who's now at Baylor. So these were pediatric oncologists. So I actually had some money left over from my NRSA which I transferred here.And then as soon as I got here, that whole program got reorganized and its number, F32 or whatever those numbers were, got changed. And for some total fluke I was able to apply for another one, which I got. It was a complete fluke. So I was supported by extramural funds during my first two years here and then – again, this was sort of a fluke – Pat Goldman, who's now Pat Goldman-Rakic, left the lab and freed up a bunch of Staff Fellow positions which Mort inherited. He was able to slip me into one of them.

Farreras: So it was '75 when you arrived at NIH, and you reapplied for the extramural fellowship…

Ungerleider: This is very bizarre, too. I was a post-doc supported by extramural funds in Mort's lab from '75-'78. Then in '78 the NRSA ran out. The National Eye Institute had organized a laboratory around Bob Wurtz, who had been in NIMH. Bob Wurtz was given a bunch of positions but he couldn't fill them all immediately. He wanted to fill them slowly so he agreed to slip me into one of these positions for two years – to hold it for him for two years, in effect – while Mort arranged to get a position for me in his lab. So I continued to work in Mort's lab, but administratively, I was in Bob Wurtz's lab. I never actually worked with Bob Wurtz.

Farreras: Wasn't Hal Rosvold still Chief of the Neuropsychology Section – and later the Neuropsychology lab – when you arrived in '75?

Ungerleider: Oh, yes, yes.

Farreras: Do you remember when he retired?

Ungerleider: 1980?

Farreras: 1980? And Mort Mishkin took over the lab in 1986?

Ungerleider: It's hard to imagine but that may be true.

Farreras: Okay.

Ungerleider: Rosvold wasn't really coming in very much in those last couple of years.

Farreras: I was going to ask you about that because when you were talking about working in Mort's lab, I wasn't sure whether it was already his – meaning the Neuropsychology lab which was created in '77 – or whether you meant you were working in Mort's section within Rosvold's lab.

Ungerleider: When I came into the lab we were a section. I arrived in September, and I think that by that winter, the section was reviewed by the BSC and the recommendation was for the section to become a lab.

Farreras: I've been trying to locate the BSC reviews but it seems that '89 is about as far back as anyone knows about, whether in digitized form or microfiche or hard copy. Do you have any idea where all of the older BSC reviews are? When did the BSC review process even begin?

Ungerleider: I don't know. When I arrived I didn't even know what a BSC was. I had no idea what this group was about, what they were coming in to do. I'm not even sure there were written reports.

Farreras: Mort [Mishkin] and Morrie [Parloff] mentioned that in the beginning a lot of it was done orally.

Ungerleider: Yes, that's right. The whole process was pretty loose. And, in fact, the impact was pretty loose. The Scientific Director was regarded as having absolute control and he could or could not listen to the advice of the BSC. But if you really wanted something more, like to become a laboratory, to get a tenured person, you had to have a BSC review. Without that, nothing happened.

Farreras: Who was the Scientific Director at the time? Do you remember? John Eberhart?

Ungerleider: Yes, but at the time I had so little interaction with the people in power. I was just doing my monkey work.

Farreras: So in 1978 and 1979 you were officially working with Bob Wurtz at the NEI but actually working in Mort's lab…?

Ungerleider: Right. And then in 1980, Mort managed – I don't know how – to get these extra positions. I think some of them came from Pat [Goldman-Rakic] leaving the lab.

Farreras: Didn't her section – Developmental Neurobiology – start in '79? It just seems that the section was converted into a lab.

Ungerleider: It must have been that when the section was converted to a lab, the three tenured faculty got their own sections: Pat, Mort, and Hal. Hal had his own section. They each had assigned to them certain resources in terms of space and positions. And thinking back on it, it must have been when Pat left that she freed up her resources, which I think Mort largely inherited and was able, in '80, to fill with a whole bunch of people. He slipped me into a Staff Fellow position. That was when Bob Desimone joined the lab, Barry Richmond, and shortly thereafter, Betsy Murray as well.

Farreras: Did they arrive as post-docs?

Ungerleider: Betsy came first as a post-doc and then became a staff fellow, but the others did not go through post-doc first.

Farreras: Can you describe the rankings for me because I've seen different terms used to describe scientists over the last 50 years in the telephone and scientific directories, such as guest workers, visiting scientists…

Ungerleider: The post-docs would be the people who usually came on extramural money. They would have applied for extramural funding.

Farreras: For a period of two to three years?

Ungerleider: Yes, for two or three years, with the idea that Mort would try then to get you a staff fellow position.

Farreras: Okay, so the staff fellow position was the next logical step, a more permanent type of position…?

Ungerleider: In those years, the staff fellow was like a tenure-track person and the position usually lasted three years after which you could be promoted to a senior staff fellow position.

Farreras: For another three or four years?

Ungerleider: Yes, and the idea was that during this six-year period your lab chief would be grooming you for possible tenure.

Farreras: When was this tenure system implemented? I was under the impression that it was very recent, that this was a recent attempt to emulate the academic tenure system but that it wasn't always there.

Ungerleider: Oh, the new tenure system happened with Varmus, so that was six years ago. The earlier system was very different. Tenured people always came from the inside, they were groomed in a laboratory and it was all done from the inside. It's true that only about 5-10% of the people who were in staff fellow positions were eventually tenured, but that was the route to tenure. There were outside letters but it was mainly an in-house thing amongst the lab chiefs and the Scientific Director.

Farreras: Oh, not just that particular lab chief?

Ungerleider: No, all of the NIMH lab chiefs. It was a very old-boy system amongst all the lab Chiefs, each of whom wanted his own guy to get tenure.

Farreras: Were these competing positions? If someone were tenured in one lab did that mean that there would be one fewer position for someone in another lab to get tenured?

Ungerleider: No, it was more like each lab chief was grooming his own people and there were alliances. This is my take on it. There were alliances formed amongst different lab chiefs about supporting each other's candidates.

Farreras: Was it possible for the other lab chiefs and the scientific director to ignore a lab chief's recommendation not to tenure someone? Or were people pretty much granted tenure if the lab chief endorsed them?

Ungerleider: Well, the system gradually became a little bit more objective. For example, I was the first one to come up for tenure in our lab. And I think that the committee wrote back to Mort and said, "Well, there's no evidence that she's independent of you. We only give tenure to people who are independent." So already by then it wasn't like, "I'll tenure your guy, you tenure my guy." They really wanted evidence for scientific accomplishments, will this person be able to set up an independent research program, because that was the new idea, that tenured people – surprise, surprise – should have their own independent research program and not just do the work of the lab chief. And I think Mort was very astute at realizing that because he really, really encouraged me to do anatomical studies, firstly because he wanted to see this kind of work done – there was very little anatomy going on in higher-order visual areas at the time – but also because he recognized that, if I had to compete for tenure, if I had to be successful, I had to carve out something for myself that he was not identified with. This wasn't a problem for either Bob Desimone or Barry Richmond because they were doing awake-monkey physiology. This is not something Mort does. That was cool. But since I came into the lab doing monkey behavior and lesions and this was what Mort did, I had better get something that belonged to me. And so, I think it really helped that Mort could argue that all of the anatomical work and all of those publications were mine – even though his name might be on them – because he didn't do that kind of work, that was my area. That really helped me in the tenure process.

Farreras: And that was after three years as staff fellow and three or four years as senior staff fellow?

Ungerleider: I think I got tenured in 1985. Everybody else followed very quickly afterwards, I think Bob and then Barry and then Betsy. I don't think there was a central tenure committee then, or maybe there was. It maybe it just went to Phil Chen for his approval. It must have gone to Phil Chen in Building 1. Have you spoken with Hazel Rea?

Farreras: I've written to her.

Ungerleider: She basically ran NIMH all those years. She would make strong people tremble. She was really a powerful woman. In the early years I really think the institute was run like a small club.

Farreras: Yes, I understand that people were hired by word of mouth quite a bit. That actually leads me to another question. Did NIMH believe that there was a need to do research in topic X, and so created Sections or labs related to X that it then filled with people, or did it hire big names and then build sections or labs around those people?

Ungerleider: I don't know.

Farreras: Okay, because it seems that these people were already here, Mort Mishkin…

Ungerleider: Mort and Rosvold were here, and then Pat came to work with Rosvold on frontal-lobe function.

Farreras: Do you know when she came?

Ungerleider: I don't know, she left in '79 or '80.

Farreras: Okay, but Rosvold had brought her in.

Ungerleider: Yes, that's right. They were doing those early lesion studies together. The only other person who was sort of permanent was Charlene Drew Jarvis, who was a post-doc or a Staff Fellow with Mort. She came from Maryland. It was at that early BSC review in '75 that they recommended a tenured position for her, and then she became tenured in Mort's section.

Farreras: In the Cerebral Metabolism Section or the Developmental Neurobiology Section?

Ungerleider: I don't know because we just called ourselves laboratory of neuropsychology.

Farreras: Oh, so the section names were just…

Ungerleider: They were for Building 1 or headquarters.

Farreras: Who came up with them?

Ungerleider: I have no idea. I guess they decided you're not going to be sections because you have separate budgets.

Farreras: So the lab chief now controlled what funding went into each section?

Ungerleider: Initially Hal controlled the budget for what was then the animal behavior section. When I came into the lab, there was bad blood between Mort and Pat, fighting over resources and cages, and Hal controlled everything, and it was not good. I think things got better when we became a laboratory. Not that I ever saw the budget, but it's certainly true that the resources were allotted to each person, and I assume that the budgets were separated for the different sections.

Farreras: Would each section receive the same amount of funding?

Ungerleider: You'd have to ask Mort. Until I became a section chief myself I never knew anything about a budget. I never saw a budget. It was very, very hierarchical.

Farreras: And the section chief would then distribute the allocated funding within that section any way he or she wanted?

Ungerleider: Exactly. In fact, after Hal was retired and Pat was gone and Bob, Barry, Betsy, and I all got tenure, we created sections in the laboratory of neuropsychology. There were several years when Mort still controlled the budget, and the sections did not have their own budget.

Farreras: When was this?

Ungerleider: When was that, in the mid-'80s?

Farreras: You must have had sections I do not know about. I have that the two first sections in '79 were Pat Goldman's and Mort's and I don't have any others listed until '92 when there was Cognitive Neuroscience, your neurocircuitry one, and Bob Desimone's behavioral neurophysiology one.

Ungerleider: Really?

Farreras: Were there any in between?

Ungerleider: Maybe not. When Pat left and Hal retired there were no sections. It was just neuropsychology. But I thought it was before…oh, I can tell you when it was, I probably have it on my CV. Ninety-two, yes, the three new sections were created in 1992. So it must have been that we all got tenure and then they put in the paperwork to create sections. That was it.

Farreras: All right. So those sections were created six years later, after Mort had taken over the lab.

Ungerleider: Right.

Farreras: So there were no sections during that time, just the lab.

Ungerleider: But during those six years, Bob and I were already helping Mort administratively run the lab. I remember that each year Bob would do the budget with Mort, and I would do all personnel actions. The lab was expanding, and so we were taking a lot of the administrative load off of Mort.

Farreras: Was this expansion as a result of it becoming its own lab or because Goldman's section disappeared and more openings became available or… How was it that there were so many more slots opening up at that time?

Ungerleider: Beats me. A lot of the technicians were inherited from Pat. She had a great technical staff. And I was the beneficiary of two people. One was her histologist, John Sewell, and the other one was her anatomist, Thelma Galkin, who did all of her microscope work. So I inherited both from Pat, because when Pat left, Mort had these two people but what was he going to do with them? So he just assigned them to me. I think it may be as part of the tenuring process. So maybe after I got tenure I was given my own staff-fellow slot to fill, and then there were people

coming in as post-docs to the different sections and…

Farreras: You were assigned staff fellows? You wouldn't go out and pick who you wanted?

Ungerleider: Oh, no, I was given a position but then I could fill it with anyone I wanted.

Farreras: So each section chief could really do his/her own hiring, it wasn't decided by the lab chief.

Ungerleider: Oh, no, uh-uh.

Farreras: Alright, so each section had control that way. When those new sections were created in '92 was that as a result of being tenured then? A way to carve your own niche within the lab?

Ungerleider: I think so, yes. And I'm not sure whose recommendation it was, whether it was Mort's recommendation, Steve Paul's recommendation – the scientific director at the time – or whether it was thought to be a good thing. I think there was a lot of discussion in the lab at the time about whether we should be separate sections or just continue the way we're going. I probably did not have strong feelings about it but I think Bob probably did, he really wanted to create a group with its own distinctive character. But even after we created these different sections there continued to be lots of interaction amongst us and cross-collaborations.

Farreras: You mean between the sections or with people outside of the lab?

Ungerleider: Between the sections. We always still saw ourselves as a laboratory.

Farreras: Was there any contact with the members of the former lab? With the psychology and psychopathology lab?

Ungerleider: I never had contact with them, ever, ever. Even when I arrived, I never did. I didn't even know who they were.

Farreras: Wow, so you're all within the same lab but still pretty isolated depending on the type of research you were doing. I know Mort mentioning that the basic sections like animal behavior, aging or perception and learning felt a bit neglected compared to the attention the clinical sections were receiving…

Ungerleider: Right. Back in the late '80s, early '90s, Mort and I were approached by Jim Haxby in the Aging Institute about testing some of the hypotheses that we had developed in monkeys on PET imaging studies in humans. So we set up a collaboration and we did these very successful imaging studies. It was pretty fluid who you could collaborate with, both within the laboratory, across laboratories, and across institutes. Mort and I did some studies with Mickey Goldberg in the Eye Institute and a couple of papers came out of that collaboration. We had some collaborations with people in Child Health [and Human Development] looking at circadian rhythms. Both Bob and I collaborated with some people in Brazil who used to come up for one-year periods of time.

Farreras: It sounds like these collaborations were always scientist-motivated. It doesn't

seem like you're getting directives from above suggesting you collaborate with certain scientists.

Ungerleider: "From above" meaning the administration?

Farreras: Yes.

Ungerleider: The only instance where I wondered what I was doing was when I bumped into and sat down for lunch in the cafeteria with a clinical investigator who said to me, "You study perception. Why aren't you in the Eye Institute? What are you doing in mental health? How is what you're doing relevant to mental health?" And our standard answer was, "Many diseases in mental health have to do with distortions in perception and memory and general cognitive impairment, and so it's really important to investigate these kinds of mechanisms or mental processes." And he said, "Well, I still think you belong in the Eye Institute."

But no one ever put any pressure on me to do things that were more clinically relevant.

Farreras: So you also got to choose what type of research you did? It doesn't sound like you had any pressure, whether from Congress or from the administration saying, "This is a very hot topic right now. We should focus our energies on X."

Ungerleider: I have reinvented myself four or five times since I've been here. I started out doing monkey behavior; then I did monkey anatomy; then I did monkey electrophysiology; and now I'm doing functional imaging in humans. And I was able to move from one to the other seamlessly, without justifying that I was capable of doing it, unlike in the extramural, outside world where you have to write a grant, propose to do something and they ask, "Well, what's the evidence this person can actually do this kind of work? And where's the pilot data?" I just did what I wanted to do. And I know when Bob came into the lab, he came in as an acute monkey physiologist. He knew nothing about behaving monkeys. And he came and said, "I'm going to set up a behaving-monkey laboratory," and he did.

Farreras: Did Mort bring him in?

Ungerleider: Yes, from Princeton.

Farreras: What criteria were used to bring people in?

Ungerleider: The same that are used now; you interview the person, you get letters, and you think the person's going to be terrific.

Farreras: So the individual first applies for the opening?

Ungerleider: This happens to me all the time, people come to me and say, "I'd really like to come and work in your laboratory," and I say, "Well, send me letters and let's see your CV and let's talk about what you would do here." And most of the time I tell the person that it's not going to be a good fit; (a) you don't have the background, you don't have the skills; you don't know anything about the brain, things like that. But if someone looks particularly good, I say, "I don't have an opening right now. I have a post-doc who'll be leaving in X months or X years,"

and I would put them in that particular position. Or I might write a memo. Ordinarily it would go to Bob – but it can't go to Bob because he's my husband – it would go to the Scientific Director. My laboratory, administratively, is under the umbrella of the [NIMH] director, [Steve] Hyman. So I would write a memo saying so-and-so wants to come to the lab, this person's really terrific for these reasons. Andrew Gerosi is leaving the lab in 10 months, but I'd like four months' overlap for transfer of knowledge and some of the routines. I would just be borrowing the position for four months and then, at that point, he would occupy Andrew's position. That's usually how it would work.

Farreras: So that would be the process if someone wanted to come in at the post-doc level, if there were a post-doc opening. What about more senior scientists?

Ungerleider: What would they come in as? Senior people don't come in.

Farreras: Oh, they don't? So the hiring is done exclusively at the post-doc level?

Ungerleider: Well, actually, I do have two – I think it's two – Staff Fellow positions under me. One I filled with what I consider to be a post-doc, but he's a neurologist – he's finished his medical training and his residency in neurology. I couldn't possibly offer him a post-doc position at $30,000 a year. So if an M.D. wants to come to the lab and I want to hire an M.D., I will put them into one of these Staff Fellow positions so they're making about $50,000-$60,000, which is the only way they can come. And my second Staff Fellow position I usually reserve for someone who comes in as a post-doc, either a foreign post-doc through the Fogarty Center or an IRTA position, which are the new post-doc positions now. And if someone wants to stay in the lab longer than three or four years after that time, I put them into one of these positions just because it's a little more senior and they make more money.

Farreras: But ordinarily these Staff Fellow positions are not available.

Ungerleider: Right.

Farreras: I see. So you have a lab chief, various sections with section chiefs who are all tenured scientists, and then within each section there are a number of senior staff, staff, post-docs and technicians?

Ungerleider: Exactly. There are no senior people in any of the sections. That's actually thought to be one of the problems with this system, that people come as post-docs and then they have to leave in three to five years for a new batch to come in and so there are no mid-level people, as opposed to the other system where some of the post-docs work their way up to more and more senior levels.

Farreras: In order to become staff and senior staff fellows?

Ungerleider: Right. And the new tenure system – instituted six years ago by Varmus – is that you recruit from the outside for tenure-track positions, you don't groom people from the inside. There is now a tenure-track process that's been put in place in which, first, you have to make a proposal to the BSC that your laboratory or

section needs a tenure-track person and what the qualifications of that person should be so no one is identified. And then the Scientific Director, having heard the recommendation of the BSC, would then approve it or not approve it, and then it has to go to the level of Gottesman [Deputy Director for NIH Intramural Research] who then approves it or not. And then a search committee is formed.

Farreras: Does the search committee consist of the lab people or of the Scientific Director and other lab chiefs, as the tenure-granting committees do?

Ungerleider: No, usually from the lab that's proposing the position, other people from the Institute, and maybe one or two people from other institutes. And then there's an open search.

Farreras: And no local people can apply?

Ungerleider: They can, but they're rarely competitive, because usually people on the outside will already be assistant professors with grants, and people on the inside are simply without grants and judged not to have established their own independent program.

Farreras: What's the incentive for an outside person to come here?

Ungerleider: Oh, God, guaranteed resources, guaranteed personnel, and funding for the rest of your life, if you're good.

Farreras: I see. When I interviewed Bob Cohen he mentioned that in the early days, in the mid 1950s, he had a very hard time recruiting people to come to NIMH because NIMH did not have the status it has today nor could NIMH pay the salaries that people were getting in academia.

Ungerleider: That's absolutely true. Jim Haxby, who now heads a section [on Functional Brain Imaging] in my lab, was recruited from Aging, and he is now leaving in September to take a professorship at Princeton because they will double his salary.

Farreras: But he'll have to start applying for grants, whereas here he doesn't have to do so, correct?

Ungerleider: Right. And NIMH supports four or five post-doc and two technical positions for him, which he will also have to get off of grant money. So just personnel salary and funding to run his program will – to duplicate what he's got here – take two to three grants, big money grants of probably half a million to a million dollars a year. Go figure.

Farreras: And that Functional Brain Imaging Section was created not too long ago, 1996?

Ungerleider: But they're going to buy him a big house, give him a big salary, send his kids to college, so from a personal financial point of view, it's a good thing to leave. But I don't think any place is…I mean, this is research heaven.

Farreras: And there is a lot of status and prestige associated with the institute as well. Do section chiefs have to propose a certain budget allocation on a yearly basis?

Ungerleider: No, the budget is pretty… I'm not sure how the budgets are arrived at. In the early days you had to really break down your budget to cover equipment purchases, travel money… Now you get a budget and it is, to some extent, based on equipment purchases each year, monkey purchases, travel money, but you can shift things back and forth, and it stays pretty much stable until the next BSC review. So when you get your budget you can expect that that will be your budget for the next four years, unless something really unusual comes up. So for this lab, at the end of the last fiscal year – we desperately needed a new server to handle all of our computing problems; we needed a lot more memory – we requested from Steve [Hyman] and Richard [Nakamura] about $100,000 that would cover the entire lab, Alex Martin, Jim Haxby, me, and all of our people. And we got that.

Farreras: What led to the creation of the four sections in '96: Haxby's Functional Brain Imaging, Al Mirsky's clinical and Experimental Neuropsychology, Martin's Cognitive Neuropsychology, and your Neurocircuitry?

Ungerleider: What happened was that NIMH decided it really wanted to make a big push into imaging, and for a while they were looking to bring in someone from the outside, like Steve Peterson from St. Louis. And there may have been – although it may have been informal – a search to find someone from the outside who could be brought in to establish a new laboratory of imaging.

Farreras: A completely new laboratory, not part of the Neuropsychology lab or the Psychology and Psychopathology lab.

Ungerleider: Right. Well, it was really going to depend on what the candidate wanted. If they wanted to be a section within Neuropsychology, that would be fine. If they wanted their own laboratory, that would be fine. It was all open to negotiations. And at one point we thought we'd recruit Larry Squire. We really felt we needed to go in the direction of human neuroimaging because everything was animal work in monkeys. And in the end – and I think this was really me and Mort pushing – it turned out that the people we were interested in recruiting were not interested in coming. Sue Swedo was scientific director at the time and we convinced her that the best thing to do would be to recruit Jim Haxby from Aging because he had the knowledge and the expertise. He had been doing PET studies for 10 years. He was more interested in normal human cognition than in aging. He wanted to give up all of his Alzheimer's work. We had already been collaborating with him. He would be the perfect person. So he was recruited, and there was a section established around him, and he got all of the resources that he wanted. And I guess it was decided, for a variety of reasons, that it would be best if he were a section within the Psychology and Psychopathology lab. I think, partly, it was thought that Haxby would bring new techniques to Mirsky that would be beneficial to his program.

Farreras: I've asked Cohen and Zahn about this because with all of the original sections pretty much having disappeared by then – except for the Animal Behavior Section

which eventually became its own lab of Neuropsychology - why not just close down that lab altogether? Apparently they didn't want to do that. They didn't know who would be appropriate to lead it, and that's how they ended up asking the BSC and had this new search where they interviewed a whole bunch of people, and Al Mirsky ended up coming. But it seemed that if it was so moribund, then why not just close the lab?

Ungerleider: I think that was the idea when they brought Mirsky in. But the problem was that I think he was doing very old-fashioned psychology.

Farreras: Meaning not incorporating the new technologies?

Ungerleider: Right, and so the idea was that if Haxby could interact with him it would be mutually beneficial because Al was studying clinically relevant issues like epilepsy, and maybe Jim could apply some of these new techniques to those studies and have a part of his research that would be more clinical. So this was viewed as a good thing. But as it turned out, there were zero interactions between Mirsky's group and Haxby's group so it didn't help at all.

Farreras: Haxby didn't want his own lab, then?

Ungerleider: He did want his own lab. And maybe a lab should have been created around him… So, in this new laboratory, Brain and Cognition, Jim would have his section and I would move my section from out of Neuropsychology.

Farreras: Yes, I wanted to ask you about that, because it seems that they were listed jointly as being part of both labs for about a year.

Ungerleider: It's possible, until all the paperwork got done. So my section moved out of Neuropsychology and into the new brain and cognition lab. Haxby was a section chief in this lab. Mirsky also became a section chief within the lab. And I recruited Alex Martin into the lab and after he got tenure he became a section chief. Alex Martin was doing a lot of focal brain lesion work in Dennis Murphy's group. He's a neuropsychologist who works with humans.

Farreras: Why "brain and cognition"? Why the change in name?

Ungerleider: Well, we had to think of a name. We mulled over this for months. This was the topic of lunchtime talk. It had to be catchy. We really wanted the emphasis to be on cognition or cognitive studies. A lot of the other names were taken already. Mort came up with a very good name – Cognitive Neuroscience – for his Section that covered everything. That should have been the name of our lab! But we just thought, well, Brain and Cognition. It was short and sweet and it could cover human work, monkey work, anatomy, imaging, lesion studies. It was all-inclusive.

Farreras: How did you feel about moving from neuropsychology out into this new lab of brain and cognition?

Ungerleider: Oh, I had a very hard time. In fact, I think that for almost two years, this office was vacant because I spent all my time over there [Building 49]. But I realized

that it was important to create more of a presence and a leadership role here.

Farreras: Were you pleased with being appointed the acting chief of this new lab?

Ungerleider: Oh, I had very mixed feelings. I was asked, so it wasn't that I had to become a lab chief. I had very mixed feelings about it because I had had so many wonderful years in neuropsychology and in Building 49, but more and more of my work was getting into imaging. There were more collaborations between Jim, Alex, and me; in the mid-'90s, we had a slew of imaging papers in which we collaborated together. More of the post-docs coming in to work with me wanted to do imaging; I got very, very few requests from post-docs to come and do anatomy anymore. Everyone wanted to do imaging. So it really seemed like this was my future and where I should be.

Farreras: It seems that a lot of the recent research on mental illness, whether it's schizophrenia, bipolar disorder, anxiety disorders, revolve around brain imaging. When do you remember seeing this shift and preference toward this new technology, which shapes many of the questions we ask and what we accept as evidence or answers for them?

Ungerleider: It began 10 years ago with PET studies, but PET is an extremely expensive technology, it's invasive – it requires radiation –, and it has to be done in a medical setting, and there are only a handful of PET centers around the world.

fMRI came on the scene five years ago with a vengeance. We were doing our first fMRI study in '94, maybe '93, and we were simply comparing PET to fMRI to see if they gave you the same result. Early on, our physicists were telling us, even though we could only collect a single slice from the brain, five years from now we'd be collecting multiple slices, and it turned out that in one year we were getting multiple slices, and six months after that, we were getting whole brain coverage. And at millimeter resolution instead of tens of millimeters of resolution, and at a temporal resolution of seconds as compared to minutes, and every radiology department across the country could have their systems upgraded to collect functional images. And now there are magnets in psychology departments, outside of medical environments. So it's just amazing. What it can't give us yet, but it may in the future, is any information about transmitters and receptors. So all of the ligand work is still being done with PET. I would say that for people who are interested in what neurotransmitters go awry in different mental illnesses, PET is still the preferred technology, or spectroscopy, where you can see blips from different molecules. But that could change, there could be another major paradigm shift as there was with fMRI.

Farreras: Do you think that it was also because you were veering toward this line of research and becoming more and more interested in this type of imaging that you were asked to head the lab?

Ungerleider: Yes, I think it was because most of my work then – and still now – has

parallel studies going on in monkeys and humans, and so the idea was that this would be the perfect way to bridge animal and human work. And so even though the other people in the lab may not be doing animal work, I was seen as the person that would bridge these two areas of investigation.

Farreras: Has your research changed a lot since you moved to the new lab?

Ungerleider: I think it's just been an evolution rather than a major shift. It's only recently that I've started doing work on attention, which I had not been doing before, but that's because some of the post-docs who came into the lab were especially interested in that and because some of Bob's post-docs were working on problems in attention. So it just fit together.

Farreras: So now, looking back on a career at NIMH…

Ungerleider: Would I have changed anything? No.

Farreras: You mentioned earlier some of the pros of a career here being that you have all the resources and personnel paid for versus, if you were in an academic setting, you'd have to be applying for grants. Can you think of any other pros or

cons of a career at NIMH?

Ungerleider: Well, even if you're well supported by the institute and have terrific resources, it is true that people on the outside, at my level, could have bigger empires than I do. I have no graduate students and I have four or five post-docs and share another two with Bob. They're working on collaborative studies with us.

But Pat Golden, for example, must have…I don't now how many graduate students she has but she must have 15 post-docs. So really successful people on the outside can have much bigger operations than people on the inside. One of our closest colleagues is Nikos Lokafidas, who is the director of the Max Planck Institute in Berlin, Germany. He is the equivalent of three tenured people at NIH in terms of resources. So, in a sense, this place is a zero-sum game, and everyone who's successful is good and is well taken care of, but if they were very successful on the outside could certainly do better in terms of salary and also magnitude of operations. And many of our colleagues are in medical schools where the teaching is minimal. You know, if you're in a psychology department, you have to teach three to four courses a year, but in medical schools maybe only a couple of lectures a year, or you can buy out teaching responsibilities with grant money. So, I think it's the grant-writing. People tell me – even people who are very successful and very well funded – that they spend a third of their time raising money, and I don't want to do that. And the idea that you are responsible for raising money for all of the people who work for you is an enormous pressure. If you don't raise it you have to lay them off. So for me, having a bigger operation is not all that appealing. I like the size of the group that I supervise. And I like the freedom to do what I want and not have to explain what I'm going to be doing. I mean, when we get reviewed by the BSC, it's mainly retrospective so it's mainly, what have you done and what are you planning to do. But if you've been successful over the four years since your last review, that carries a lot of weight. In the grant system, it carries some weight, but not that much. It's like, what is the document you've produced that tells us what you're going to do?

Farreras: So productivity is measured by how many papers you've published and how many presentations you've given?

Ungerleider: Yes, papers.

Farreras: So more than how many you publish how often they're cited in the literature, as a measure of its impact or importance on the field?

Ungerleider: Absolutely. And which journals.

Farreras: Meaning if they're published in first-tier journals or…

Ungerleider: Right.

Farreras: Are there any fixed criteria that you have to…

Ungerleider: No, because awake-monkey physiology takes years to produce a paper while imaging works faster. So when my imaging post-docs leave the lab, they have three or four papers under their belt. When our monkey people apply for jobs, they have one *Journal of Neuroscience* paper. And I think we're also judged a little bit on where our post-docs wind up. Do they get good jobs?

Farreras: You mean whether they place at research one institutions?

Ungerleider: Yes.

Farreras: I see. How was a career as a female scientist at NIMH been? You had mentioned Sue Swedo's report…

Ungerleider: Yes, so it turns out as a result of that report that I was grossly underpaid. But that was adjusted and is now fine. I don't know if I am a good representative of what it's like to be a woman at NIMH. Mort was a very, very benevolent lab Chief when I was in his group. He gave me a lot of resources, both people to work with and technical support. He did everything he could to make my science work. And he promoted me. He worked with the system to get me tenure and to get me my own section, and he's just been great, really great. I use him as a model in mentoring the people who are under me. He was never the sort of person who said you have to be in at such-and-such a time. In Mort's lab you put in the hours if you wanted to because that was what it took to be a good scientist. So, for me,

this system has been ideal, but I think I really did have a special lab Chief.

Farreras: So it depends on where and who you're working for?

Ungerleider: Yes, because if you look around, there aren't that many women. There are really not that many women. I sat on the NIH Tenure-Wide Committee for one or two years and a lot of the women who were coming through – and there weren't that many who were being recommended for permanent positions – had been very badly supported. They'd really, really not been given resources to develop their own programs. Some of the women whom we reviewed barely had a post-doc to work with them for their years. This is bad. And the only thing we can recommend to some of them is – I forget what they're called now – a staff scientist appointment, which is not an independent appointment. You don't get independent resources as a Staff Scientist; you're assigned to a tenured scientist. But it was great for me.

Farreras: Well, is there anything that I'm missing that you would like to add? No? Ok, well let me thank again for devoting all this time to this oral history.